EARLY SELECTION IN HUNGARY

A POSSIBLE CAUSE OF HIGH EDUCATIONAL INEQUALITY

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ABSTRACT

The paper shows that two special types of academic tracks, which select students at age 10 and 12, increase test score gap between high and low status students in Hungary. Generalizing this finding I conclude that early selection causes the already not-small educational inequality of opportunity in Hungary to grow. It is shown that higher status students are more likely to attend these early-selective academic tracks, even if previous test scores are controlled for. Also the value-added of the longer 8-year-long early-selective track is higher than that of the 6-year-long academic track, which in turn has a higher value-added than the general track. Taking everything together it seems that early-selective tracks have higher value-added, but the size and the significance of this effect varies across cohorts and subjects. The results are in line with the expectations derived from the literature: early selection is assumed to have an inequality increasing effect due to different teacher quality and also due to different peer effects between different tracks. It is also shown that early selection is not a Pareto improvement as the average performance is concerned.

1. INTRODUCTION

Age of selection, likely the best proxy for comprehensive schooling, is strongly associated with inequality of opportunity in a cross-country analysis. The earlier children are selected in an education system, the more likely that the family background will associate strongly with their literacy scores (Horn 2009). Naturally, this does not mean that all education systems that select late are equal. However, its opposite that early selection implies inequality is commonly acknowledged by the scientific community (Ammermueller 2004; OECD 2005; Pfeffer 2008). Nevertheless, there are very few empirical papers that could show the causal link between early selection and some forms of inequality (but see Hanushek and Woessmann 2006 for a diff-in-diff analysis). Some, on the other hand, could convincingly show the positive effect of increasing the age of selection on inequality (Meghir and Palme 2005; Pekkarinen, Uusitalo, and Kerr 2009).

This paper will contribute to this line of research by comparing the efficiency of two types of academic tracks in Hungary to each other and to the other tracks. A unique feature of the Hungarian system allows for a comparison of the effectiveness of these two types of tracks that are very similar in many aspects, but differ in their age of selection, and also to compare both to the other tracks.

The following section introduces the problem of early selection and its hypothesized effect on inequality. Then I shortly introduce the Hungarian public education system, including the early-selective tracks, arguing that Hungary is an exemplary case for a test of the effect of selection on inequality. The next section presents the utilized dataset, the National Assessment of Basic Competencies (NABC). The bulk of this paper is the empirical analysis with a concluding final section.

2. EFFECTS OF EARLY SELECTION ON INEQUALITY

Meghir and Palme (2005) and Pekkarinen, Uusitalo and Kerr (2009) have shown in the case of Sweden and Finland, respectively, that comprehensive education reforms help to equalize the system by increasing intergenerational mobility. The authors have demonstrated that although increasing the age of selection decreases inequality it might not be beneficial for the higher status families, who tend to lose not only in relative but also in absolute terms. These reforms took place in the 1950s in Sweden and in the 1970s in Finland. Both countries abolished tracking and both imposed national curriculum on schools and lengthened compulsory schooling to 9 years from 7 or 8. Meghir and Palme (2005) demonstrated that the reform increased both the attainment and the later earnings of children with lowly educated parents. At the same time, the reform also decreased the earnings of those with highly educated parents. Pekkarinen, Uusilato and Kerr (2009) tested the effects of the Finnish comprehensive reform and concluded that it had only a small but an overall positive effect. It significantly reduced intergenerational income elasticity for boys, and it increased intergenerational income mobility. The novelty of these studies is that they could test the causal effects of an educational reform utilizing quasi natural experiments.

But what are the reasons behind this effect? Why would a higher age of selection decrease inequality? The most likely reasons are, among others, that teachers matter and peers matter. More precisely: since teachers matter, selecting students into homogeneous groups *and* letting the quality of teachers differ across groups affects the performance of the students differently in the selected groups. Tiebout (1956) in his seminal paper has shown that "If consumer-voters are fully mobile, the appropriate local governments, whose revenue-expenditure patterns are set, are adopted by the consumer-voters" (p. 424). By replacing "consumer-voters" with "students" or with "teachers" in the Tiebout paper, one arrives at the observation that students and/or teachers are non-randomly distributed across schools. If there are differences in teacher quality between schools, this will affect the mean performance of the students differently between schools. This is precisely the starting point of Rivkin, Hanushek and Kain (2005), who test this assumption. They test the effects of teachers on students, using a large matched teacher-student data from Texas. They arrive at the unsurprising conclusion that "teachers, and therefore schools matter importantly for student achievement" (p. 449). Another way of looking at teacher quality, besides rich teacher-student matched datasets, is to qualitatively assess the reasons behind superior education performance. The McKinsey&Company (2007) looked at the best PISA[1] performers and concluded that the most important reason behind the quality of these education systems is the high quality of teachers. The best systems "get the right people to become teachers" because "quality of an education system cannot exceed the quality of its teachers" (McKinsey 2007).

Peers can have an important effect as well. Several papers have attempted to identify the effects of peers on student outcomes. Although this issue is loaded with methodological problems (see Manski 1993) some have especially convincingly shown that peers indeed matter (Sacerdote 2011). Hoxby (2000) as well as Hanushek, Kain, Markman and Rivkin (2003) have shown in the case of public education that peer effects are important. Higher achieving schoolmates can improve others, but these effects are likely to be reciprocal: less bright peers can be a hindrance. Thus again, in a system, where students are selected into homogeneous groups, peer effects could excel differences between schools.

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[1] PISA - Programme for International Student Assessment (see e.g. OECD 2001, the first report)

Teacher and peer effects are only two examples why early selection might increase differences between students (there could be other possible reasons, for instance, curricular differences between tracks), but these two alone gives enough rationale to take a look at whether selecting students early increases the gap between students.

3. THE HUNGARIAN PUBLIC EDUCATION SYSTEM

Hungary is an obvious choice for a test of the effect of early selection on educational inequality. Firstly, inequality of opportunity in the Hungarian education system is especially high. The variance in reading performance explained by family factors was the highest among all PISA participant countries in 2009 (OECD 2010). In other words family matters a lot in how students perform in schools. Secondly, the education system is rather selective. The Hungarian system selects children quite early, first at age 10. It has not always been like that. Before 1989 the system was a typical "soviet" system, with 8 years of general training and three types of secondary tracks, where students could study after age 14. There were two vocational tracks, a relatively more academically oriented vocational secondary/technikum (*szakközépiskola*) and a more practical vocational/apprentice training (*szakmunkásképzés*) track, and an academic track (*gimnázium*). While these three tracks continue to exist today,[2] there are two additional types of academic tracks, the 8-year-long and the 6-year-long academic tracks (see figure A.1 in the appendix). I will call these latter two, somewhat suggestively, *early-selective academic tracks*. The 8-year-long academic (8-yr-ac) track selects students just after 4th grade, at age 10. Two years later, after 6th grade at age 12, the 6-year-long academic (6-yr-ac) tracks select another group of children. And finally after 8th grade, at age 14, everyone must choose some secondary level track to continue her/his studies until the age of 18. Only around 3-4% of the whole cohort leaves general schools at age 10 and an additional 4-5% at age 12 to enter the 8-yr-ac and the 6-yr-ac, respectively (see table 2 and 3 below). The introduction of the early-selective tracks was gradual. Their establishment was possible between 1989 and 2000, and most of them were established between 1991 and 1997 (see a more detailed description in Horn 2010). There are several areas in the country where no such tracks were established (see figure A.2 in the appendix). This paper utilizes this unique multi-level selective feature of the Hungarian system, as well as the spatial variation in the early-selective tracks to test the effects of early selection on inequality.

My goal is to show that the causal effects shown by Meghir and Palme (2005) and Pekkarinen et al. (2009) go backwards as well. Namely, to examine whether decreasing the age

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[2] The apprentice training has been renamed and reformed to vocational training (*szakiskola*), but that process is outside the scope of this study.

of selection would increase educational inequality and whether such a change would help higher status families while hurt others. There are but a few such examples in the history of educational reforms that opted for decreasing the age of selection, Hungary being one of them.

A complementary study of the Nordic cases would look at the long term effects of the introduction of early-selective Hungarian academic tracks. Unfortunately, there are quite a few problems with the execution of such a study. The most important is that while the Nordic reform was supply driven – the central government dictated the terms – the introduction of the early-selective tracks in Hungary was demand driven. One of the most important reasons for establishing an early-selective academic track in a given settlement was the demand from the local community. Local citizens – parents, teachers, maybe the school itself – or the church could lobby for such a track. When and where an early-selective track was introduced depended on the local community. The post-transition central governments allowed these educational changes, but did not initiate them (see Horn 2010). Unfortunately this factor is likely to introduce endogenity problems into a test of the effects of early selection. The second problem is that while the Nordic reforms took place in a relatively short period in time, hence identifying the before and after cohorts were relatively simple, the Hungarian transformation took several years. And finally, to address the long term effects of the decrease of the age of selection would require a large dataset with some outcome measure (like earnings or income, as in the cited studies) and several additional variables so that people are observed before and after the early selection was introduced. Because the first cohorts entering the early-selective tracks have just entered the labor market, currently no such study can be done.

Nevertheless, I can address related questions that shed some light on the original puzzle. While I cannot study the effects of the “reform” in general, I can take a look at the recent “micro-effects” of these early-selective tracks. That is, I can test whether children entering early-selective tracks gain by doing so, and whether others lose because of this.

This paper will address the following three questions.

(1) Are early-selective tracks status-selective?
(2) Do early-selective tracks have a higher value-added?
(3) Do others lose because of the early selection?

If answers to the these questions are affirmative – thus we observe that early-selective tracks are “better” while other tracks are “worse”, and that higher status families tend to benefit from this while others lose – then one could argue that the Hungarian institutional change in the early 1990’s might have done just the opposite of what Mehir and Palme (2005) and Pekkarinen et al. (2009) have observed in the Nordic states: increase inequality by providing better education for the higher status citizens.

4. The NABC database and descriptive statistics

The National Assessment of Basic Competencies (NABC) is a standard based assessment designed similarly to the OECD PISA survey, but conducted annually in May.[3] It measures reading and mathematical literacy of the 6th, 8th and 10th grade students and it is standardized to a mean of 1500 with standard deviation of 200. The mathematics and the reading scores are standardized not only within but also across years. The average score of 6th grade students in 2008 was 1500 (both in math and reading) and each cohort and class is measured to this 2008/6th grade cohort. For instance if the average mathematics score of the 6th grade students in 2010 is 1550, this means that this cohort's average mathematical literacy is quarter of a standard deviation higher than that of the two year older cohort. Similarly, one can compare the scores across years within cohorts.[4]

Table 1 shows who and when was measured within the NABC survey. There are several explicit goals of this assessment. First is to provide more detailed and more frequent feedback for the educational policy than the international surveys. The second is to offer a tool for the school providers and schools themselves to improve. The third goal is to set the grounds for a future accountability system and provide higher transparency. In addition to all this, it offers invaluable data for the researchers to address education related puzzles. Unfortunately up until 2008 the database could only be analyzed on a cross sectional basis, because it had not contained permanent student level identification numbers. From 2008 onwards the biannual datasets are linked on the student level, thus from 2010 more detailed analyses are possible.

(table 1 around here)

In addition to the mathematics and literacy test scores the database contains extensive information on the student background and on the school site. These questionnaires resemble that of the PISA survey.[5]

This paper uses data on the 2008/6th grade and the 2008/8th grade cohorts. All of the students from these two cohorts were observed two years later, in 2010. Table 2 and 3 shows

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[3] See Hermann and Molnár (2008) for a more detailed, Hungarian language, description of the NABC database.

[4] See the description of the score generation procedure here (in Hungarian, accessed 07-01-2011): http://www.kir.hu/okmfit/files/Valtozasok_az_Orszagos_kompetenciameres_skalaiban_vegleges.pdf

[5] The national and school reports, the questionnaires and all related documents can be downloaded in Hungarian from the http://www.oh.gov.hu/kompetenciameres-6-8-10/orszagos-kompetenciameres website.

the dropout/repeat rates as well as the number of students changing tracks during these two years. Between 6th and 8th grade approximately 6,2% of the cohort repeats year(s) or drops out. This number more than doubles between 8th and 10th grade. There are also a small but significant number of students that change tracks between 6th and 8th grade. Approximately 0.2% of the 3.6% leaves or enters the 8-yr-ac during these two years. A somewhat larger fraction leaves the early-selective tracks between 8th and 10th grade. Students repeating years, dropping out or changing tracks are certainly not typical. In order to eliminate the bias these students might generate I focus my analysis on only those students that finish two academic years within two years and do not change tracks.

(table 2 and 3 around here)

The variable indicating the family status of the students is the socio-economic status (SES) index. This is generated similarly to the economic-social and cultural status (ESCS) index of the OECD PISA studies. The SES index is a 0 mean 1 standard deviation factor of three factors – just as in the PISA database – parental education, parental occupation and home possessions. The parental education is the highest parental education in years. The parental occupation is a standardized factor of the father's and the mother's employment status. While the index of home possessions is a factor of the following variables: number of rooms at home, number of mobile phones at home, number of computers at home, number of cars at home, number of bathrooms at home, number of books at home, have internet connection at home, have own books at home, have own table at home, have own room at home, have own computer at home. Generally I have used background data from 2008 (if conflicting data was provided), but imputed the SES variable with information from 2010 if data from 2008 was missing.

Table 4 and 5 below shows some very basic descriptive statistics of the different track types. It is obvious that the early-selective tracks (both the 6-year-long academic tracks and the 8-year-long academic tracks) have higher status student composition and higher average test scores. That is, they have a selected group of students. It is, however, not at all obvious whether students in these tracks are of higher status, because schools tend to select higher achieving students through entrance exams (which are typical in these tracks), who tend to be of higher status, or that there are other, status dependent barriers to entry as well (fees, distance from home, discrimination… etc.).

(table 4 and 5 around here)

5. ARE EARLY-SELECTIVE TRACKS STATUS-SELECTIVE?

The conditional student composition of the early-selective tracks shows the status selectivity of the early-selective tracks. By controlling for previous test scores the coefficient of the SES index on track choice shows how important status is. I could not look at the 8-yr-ac track choice in 6th grade, because controlling for the 6th grade test score would introduce endogenity problem: students in 8-yr-ac tracks have already studied there for two years, hence 6th grade test scores are affected by the tracks, the depended variable in our estimation. On the other hand, point estimates of the 6th grade test scores in the 6-yr-ac track choice regression are unbiased. We observe students just before they enter 6-yr-ac tracks. Hence I report only the 6-yr-ac track regressions (students in 8-yr-ac tracks are dropped).

(table 6 around here)

The estimation procedure is a simple logit regression with school-site clustered standard errors. In all models shown in table 6 above, the dependent variable is the 6-yr-ac track dummy (6-yr-ac=1 and general track=0), and the independent variables are: SES, test score and gender. Test score here is a simple mean of the 6th grade mathematics and reading test scores. This variable is standardized to mean 0 and 1 standard deviation, so that the coefficients of the SES and the score can be compared. All variables are on the individual level.

The results indicate that status matters. Indeed the uncontrolled odds of an average student with one standard deviation higher SES to go to 6-yr-ac track is almost 3 times higher (model 1). Test score matters more: the odds of students with one-standard deviation higher test scores are almost 4 times higher than of a student with average scores (model 2). Nevertheless including both in the regression drops the size of both effects, but both still remain highly significant (model 3). Taking into account spatial barriers, the distance from home to the nearest 6-yr-ac track, does not significantly change the coefficients (model 4) neither does the interaction of SES with score, which is also insignificant (model 5). Including 6th grade school site fixed-effects – i.e. taking into account the general school track effects, where students came from – increases the significance and the size of (almost) all coefficients (model 6). This is of course a very restrictive model. It compares those students, who are in the same general school in 6th grade. This can only be done in those schools, where at least one student has entered a 6-yr-ac, which greatly restricts the sample.[6] Not only the score and the SES effects are highly significant, but also their interaction: it seems that status matters less if one has high test scores

[6] Note however that running model 5 on the restricted sample (model 7) results is virtually unchanged coefficients, which indicates that the altered results of model 6 are not due to the changed sample size.

or similarly, test scores matter less if one has high family status. In short, one needs at least one of these – status or skills – to get accepted.

So it seems that status matters. Even if the reasons behind this result are unclear – whether it is due to higher fees, some other income related barriers, pure discrimination of low status students or else – higher status students more likely attend early-selective tracks, *ceteris paribus* skills.

6. DO EARLY-SELECTIVE TRACKS HAVE A HIGHER VALUE-ADDED?

Based on the summarized literature I expect that 8-yr-ac and 6-yr-ac tracks have superior performance compared to general tracks. Similarly, 8-yr-ac tracks should perform better compared to the 6-yr-ac track, because students have two additional years of higher quality teachers and peers.

In order to see the track differences I estimate a simple OLS regression for the 2008/6th grade cohort (table 7) and for the 2008/8th grade cohort (table 8). The dependent variable is the 2010 reading and mathematics test scores. I standardized the test scores to mean 0 and standard deviation 1, within cohorts and years. Control variables are the 2008 standardized test scores, the SES index and the gender of the student. All standard errors are clustered on school-site level. Variables of interest are the general track and the 8-yr-ac track dummies. 6-yr-ac is the reference; hence differences between early-selective tracks and the general track and differences between the two early-selective track types are easy to see.

In an ideal world assessing the value-added of the different tracks the following production function could be estimated:[7]

$$y_{it} = \alpha_1 + \beta_1 X_i + \gamma_1 Z_t + \delta_1 IQ_i + \varepsilon_1 \qquad (1)$$

where y is the test score of the student, X covers all individually observed time invariant characteristics (here SES and gender) that might affect the test score, Z is the track specific variable that could vary with time (here track type), and IQ is an unobserved time invariant individual characteristic that most likely have an important effect on the test score (here an omitted variable). α, β, γ and δ are the parameters and ε is the error term. The subscript i stands

[7] see Todd and Wolpin 2003 or Dolton 2002 for a more detailed review of the production function approach

for the individual and t for time. To get rid of the obvious omitted variable bias I utilized the panel structure of the data and estimated the following equation:

$$y_{it} = (\alpha_1 - \alpha_0) + (\beta_1 - \beta_0)X_i + (\gamma_1 - \gamma_0)(Z_t - Z_{t-1}) + (\delta_1 - \delta_0)IQ_i + \theta y_{i(t-1)} + (\varepsilon_1 - \varepsilon_0) \qquad (2)$$

where 0 subscripts indicate parameters in time t-1 for the same equation as in (1), and θ is a parameter for the t-1 test score.[8] Assuming that the effect of IQ on test score is unchanged through time (i.e. $\delta_1 - \delta_0 = 0$) the equation can be simplified to:

$$y_{it} = \alpha + \beta X_i + \gamma(Z_t - Z_{t-1}) + \theta y_{i(t-1)} + \varepsilon \qquad (3)$$

where $(\alpha_1 - \alpha_0) = \alpha, (\beta_1 - \beta_0) = \beta, (\gamma_1 - \gamma_0) = \gamma,\ and\ (\varepsilon_1 - \varepsilon_0) = \varepsilon$. Thus the estimated parameters indicate the difference between the effects of the independent variables on test score levels in different points in time. For instance the coefficients of the 8-yr-ac in table 7 below indicate how much better the 8-yr-ac track students perform compared to the 6-yr-ac in 8th grade relative to the difference between the two tracks in 6th grade.

The first two models in tables 7 and 8 show the uncontrolled differences between tracks. General school students perform approximately 0.7-0.75 standard deviations (s.d.) lower than 6-yr-ac students, who are approximately 0.1 s.d. below 8-yr-ac students in 8th grade.

Controlling for previous test scores reduces the gaps drastically between 6-yr-ac and general tracks (models 3 and 4). Differences in reading literacy remain significant (0.144 s.d.), but the gap in mathematics disappears. Taking family status into account further reduces the gap in reading (0.03 s.d.) and reverses the sign in mathematics (model 6): it seems that general tracks have a higher value-added in mathematics compared to the 6-yr-ac tracks (models 5 and 6). However, early-selective tracks are more likely run by the capital (Budapest), by the counties or by foundation and private providers. It is reasonable to think that there are substantial differences between schools run by different education providers.[9] Since I am interested in the difference early selection creates, *ceteris paribus* policy differences, I control for the educational provider. In models 7 and 8 the difference in math between the general tracks and the 6-yr-ac fades away.[10] The strongest test of selection effect is to look at between track differences within schools. Including the school site fixed-effects in the regression (models 9 and 10) shows these differences. The advantage of the 6-yr-ac is significant in reading, but not significant in math

[8] Note that if θ=1 the equation is a simple difference between equation (1) estimated in t and t-1.

[9] The system is highly decentralized: curriculum, financing as well as personnel policy depends greatly on the provider.

[10] It would, of course, be interesting to see why there are such differences between the different providers, but this question falls outside the scope of this study.

compared to the general tracks. The performance differences between the two types of early-selective tracks disappear. Note however that there are only four schools in the country where both early-selective track types are present. Thus differences between the two early-selective tracks in the last two models probably do not reflect the true differences between these tracks. On the other hand, out of the 149 schools, where 6-yr-ac tracks operate, 138 have general track as well (see table A.1 in the appendix). Thus these school site fixed-effect models might offer a stronger test for the difference between general and 6-yr-ac track effects than the previous models.

Differences among the two early-selective track types are not significant between 8th and 10th grade (see table 8). On the other hand, differences between 4-year-long academic tracks (normal *gimnázium*, henceforth 4-yr-ac) and the early-selective academic tracks are significant and sizeable in all specifications. Early-selective tracks have about 0.1 s.d. higher value-added in reading and 0.2 s.d. higher value-added in mathematics.

In sum, the differences between 8-yr-ac and 6-yr-ac remain significant and sizeable in both subjects in almost all specifications between 6th and 8th grade, but not between 8th and 10th grade. 6-yr-ac seem to perform better than the general track in reading but not in math between 6th and 8th grade, but 6-yr-ac does better in both subjects between 8th and 10th grade than the 4-yr-ac.

Although these results comply with the expectations outlined in the literature – that selective tracks fare better than the general ones – the OLS specifications are likely to be biased. Track variables are likely to be endogenous, since the general track as well as 8-yr-ac track variables affect not only the 8th grade test scores, but also the 6th grade test scores. Or in other words if $Z_t=Z_{t-1}$, the track variable in the estimated OLS regressions is endogenous.[11]

(table 7 and 8 around here)

6.1. Endogenity problems

In both sets of regressions I observe students few years after they have been accepted to their track. In 6th grade students in 8-yr-ac and students in general tracks have been in their track for two or six years, respectively. Similarly in 8th grade students in 8-yr-ac and in 6-yr-ac have been attending the given track for four or for two years. Hence these tracks are likely to

[11] Note that in the regressions I assume that the track variable changes, if it is not changed, Z_t-Z_{t-1} would simplify to 0 in equation (3).

have an effect on the test score in all grades. Therefore the estimated track coefficients tend to be biased.[12]

The sign of the bias is not straightforward. If school effects are assumed to be exponential (assuming increasing returns) observed effects tend to overestimate the real effects, however with diminishing returns, real effects might be larger than the observed. Close to linear effects diminish the size of the bias.

In order to get a glimpse of the unbiased differences between tracks I introduce an instrumental variable: distance from home to the nearest 6-yr-ac and 8-yr-ac.[13] These two variables will provide the exogenous variation in the model. I argue that distance from home to the nearest early-selective track does not have a direct effect on the test score, but it signals the chances to get accepted to an early-selective track. Any observable association of distance and test scores are due to the different chances in attending early-selective tracks. As I have shown before, distance from the nearest academic track has a negative effect on attending the given track type (see table 6). However distance is unlikely to have an effect on how students perform in the given track, assuming that s/he has already been accepted. Figure A.2 in the appendix shows the regions in the country, where no early-selective tracks are available. The average student lives 9.9kms away from the nearest 6-yr-ac, and 12.5kms away from the nearest 8-yr-ac. Students already in 8-yr-ac live on average 3.3kms away from the school (assuming they went to the nearest 8-yr-ac), and 6.8kms away from the nearest 6-yr-ac. Similarly, students in 6-yr-ac live 3.5kms away from their school, but 9.7kms away from the nearest 8-yr-ac. Students in general tracks have to travel 10.3kms and 13kms to the nearest 6-yr-ac or 8-yr-ac, respectively.

The exogenity of the distance can be challenged on two grounds. Firstly, it is reasonable to assume that parents are willing to transport their children further away if the child is smart (the "Alchian-Allen effect", see below), which means distance correlates with test scores. Second, distance from an early-selective track might correlate well with the average level of socio-economic status, which in turn might associate with average level of teacher quality of the general track that has an impact on student test scores.

The first problem is a "shipping the good apples out" problem (cf. Alchian and Allen 1964). In their *University Economics* book Alchian and Allen explain why it is more rational to ship higher

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[12] Note that the coefficients of the 6-yr-ac (reference) in the 6th to 8th grade panel, and the coefficients of the 4-yr-ac, technikum and vocational training tracks are unbiased, because we observe students before they enter these tracks, i.e. $Z_t \neq Z_{t-1}$

[13] Distance is measured as linear distance from the center of the settlement where the student lives to the center of the school's settlement.

quality and thus more costly products further away, if the cost of transportation is the same for all goods. Intuitively, the high quality good is relatively less expensive in distant places if lump-sum transportation cost is added. The same logic might apply for school choice as well. A parent might invest more in transportation, if s/he knows that her/his child is smarter, and thus expects greater returns from a better school. However appealing this theory is, back of the envelop estimations do not support the hypothesis (not shown here). The 6th grade test score and distance from home to the nearest early-selective track does not associate significantly for students within the same track. That is those students, who come from further away, do not have higher previous test score compared to other students in their own school who travel shorter distances. This lack of relationship can be due to three separate reasons. Firstly, parents might take their child further away not when the kid is smart but when they assume that s/he can *improve* faster in a better school, ceteris paribus their 6th grade test score and family status. Secondly, the instrument is not the distance from home to the actual track, where the student studies, but the distance from home to the nearest early-selective track. Thus this variable indicates less the Alchian-Allen effect (Do parents take their child further?) and more some physical barrier (Do they have a car? Is there a bus service?). Hence effect of the instrument on test scores is not important. Finally, simply the result might indicate that the Alchian-Allen effect does not hold here. If the first reason is correct, the instrument might not be as good as I hope. But if one believes the second or third argument the instrument is fine. Unfortunately I could not test which of these possible reasons might be true.

The second possible problem of the instrument is that the distance from the nearest early-selective track correlates with the average family status of the students. The early-selective tracks have been established mainly due to local demand (Horn 2010). That is, mainly higher status parents, or active local religious communities demanded the establishment of the early-selective tracks. This correlation between distance and status in itself would not be a serious issue, besides increasing standard errors, since the status of the students is controlled for. However average status tend to correlate with teacher quality on the aggregate level – I assumed that teachers are just as segregated as students, due to the Tiebout (1956) logic (see Varga 2011 for empirics on Hungary) – and hence we can assume that distance from the nearest early-selective track also associates negatively with teacher quality. Nevertheless this is only problematic if we assume that teacher quality affects the ability of students to improve faster. For instance, if we assume that students educated by high quality primary school teachers will improve faster between 6th and 8th grade (ceteris paribus their family status and 6th grade test score) then the instrument might correlate with the 8th grade test scores.[14] But if we assume only that good primary school teachers have an impact on the level of the test scores, the IV estimates are unbiased. Unfortunately, again, testing for the functional shape of the teacher effect is not possible with the available data.

Using these two instruments to predict the probability of entering 8-yr-ac *and* general tracks in one regression produces highly correlated track probabilities. Thus I split the sample into two to gain stronger estimates and to avoid multicollinearity in the pooled model. First, I limit the sample to only those who attend any of the two early-selective tracks. Second I test the difference between the 6-yr-ac and the general track between 6th and 8th grade and the 6-yr-ac and the 4-yr-ac between 8th and 10th grade.

Table 9 and 10 below shows the 2SLS IV estimation on a sample of all settlements, where either 6-yr-ac or 8-yr-ac is available, and on a sample where both track types are available.[15] The first stage estimations show that distance is still a strong predictor of track choice, even within this restricted sample. Distance from the nearest 8-yr-ac decreases the chance of entering an 8-yr-ac track, while distance from the 6-yr-ac increases the same probability. The 2SLS estimation does not contradict the OLS results. 8-yr-ac tracks have a greater value-added in math between 6th and 8th grade, and in reading between 8th and 10th grade. These results underline the expectation that two year advantage in a selected track further increase the differences in test scores between students.

(table 9 and 10 around here)

The second set of analysis concerns the 6-yr-ac and the general track (tables 11 and 12). As I have shown before, these two track types are usually operated within the same school. This allows for a within school estimation of the track effects, as above, using the instrumental variable. Because we test differences within school, the highlighted problems with the instrument are minimal. On the other hand due to the same reason the instrument is also much weaker. In fact between 6th and 8th grade the instrument does not work, it does not explain within school track choice at all. Similar students coming from greater distances entering a given school are just as likely to enter the general track as the 6-yr-ac track. Thus the school-site fixed-effect instrumental estimation is not conclusive. The instrumental estimation without school-site

[14] An alternative logic would be that students educated by bad teachers will eventually catch up, being freed from the retracting force, cf. convergence to the mean.
[15] This later sample eliminates the concerns about aggregate level status differences between settlements.

fixed-effects on the other hand underlines the expectation that 6-yr-ac tracks have a greater value-added, but the difference is significant only in reading.

Looking at the difference between the 6-yr-ac and the 4-yr-ac, the effects are rather weak. The instrument seems to work better for this set of secondary school students. That is, students living further away from a given school with both tracks are less likely to attend 6-yr-ac than 4-yr-ac.[16] However instrumented track effects are not significant in the fixed-effect regression. Without the fixed-effects 2SLS results are similar to the OLS results. Students in 6-yr-ac tracks perform better compared to students in 4-yr-ac tracks, but only in math. Shortly, it seems that 6-yr-ac tracks are a slightly better in reading between 6th grade and 8th grade and in math between 8th and 10th grade, but the results are not robust to the fixed-effect specification.

Taking everything together it seems that early-selective tracks have higher value-added, but the size and the significance of this effect varies across cohorts and subjects. Longer 8-yr-ac tracks have superior performance than 6-yr-ac tracks in both of the observed periods, but the significance of the effect varies by time and subject. Also both of these early-selective tracks tend to outperform the "majority" but only in reading between 6th and 8th grade, and in math between 8th and 10th grade. So the results suggest that early selection increases inequalities. Not only the early-selective tracks fare better through the observed four grades in both subjects than the majority tracks, but the two virtually identical early-selective tracks also show differences due only to their different age of selection. But one question still remains. Even though it seems that early selection increases differences between students, the society as a whole could benefit from the early-selective tracks, unless students left in general schools lose because of this early selection.

(tables 11 and 12 around here)

7. Do others lose?

The spatial variation in early-selective tracks allows for a comparison of those students, who remained in general tracks in regions where early-selective tracks are available and those, who had effective spatial barriers to entry. As above I assume that performance differences between tracks are due to differences in teacher quality or differences in peer effects. I hypothesize that in areas where 6-yr-ac or 8-yr-ac tracks "cream skim" the best students those left in general schools have a relatively lower increase in literacy scores. This is because in

[16] Maybe parents consider the cost of the additional 2 years of transport.

schools, where the best students *and* teachers are unable to opt out from the general tracks, the average teacher and peer effect is higher.

I use propensity score matching to test the differences between general track students in areas with early-selective tracks (treatment) and without such tracks (control). Propensities are calculated using the above utilized individual characteristics. 6th grade math and reading score, SES and gender. Treatment group will be those who live in a settlement with an early-selective track. That is, I assume that students living near to early-selective tracks have no serious barriers to entry. Control group will be those who live at least 15 or 20 or 25 kilometers away from these tracks. These distances are underestimations of the real distances that have to be traveled, because they are measured as straight lines between settlements. For the estimation of the average treatment effects I use nearest neighbor as well as stratification matching. Table 13 below shows the number of treated and the number of controls in each specification, and table 14 shows the average treatment effects and their standard error.

(tables 13 and 14 around here)

It seems that students lose in mathematical literacy between 6th and 8th grade due to the early selection, but effects are not significant in reading literacy. In other words, general track students in areas where any of the two early-selective tracks is available (treatment) are doing worse in mathematics than students in areas without early-selective tracks (control). The same effect is not apparent in reading. The treatment effect is not very sensitive to the definition of treatment and control groups, although when comparing students outside the 25 km radius to the treatment group the effects become less significant; but this is probably due to the relatively small number of controls.

8. Conclusion

This paper shows that two special types of academic tracks, which select students at age 10 and 12, increase the test score gap between students of different social background in Hungary. Generalizing this finding I conclude that early selection in education increases inequality of opportunity.

Comprehensive education reforms in the Nordic countries have been shown to decrease the inequality of opportunity (Meghir and Palme 2005; Pekkarinen et al. 2009), but we know little about the reverse effects: whether decreasing the age of selection in a previously comprehensive country would produce increasing inequalities. Hungary has done just the opposite of what the Nordic states have done. Hungary decreased the age of first selection from 14 by introducing two types of early-selective tracks that select at ages 10 (8-year-long academic track) and 12 (6-year-long academic track). This paper looks at the effectiveness of these two types of early-selective tracks and shows that higher status students are more likely to attend these tracks, even if previous test scores are controlled for. Also the value-added of the longer 8-year-long academic track is higher than that of the shorter 6-year-long academic track. Similar, but less significant are the differences between the 6-year-long academic track and the general track. 6-year-long academic track performs better in reading between 6th grade and 8th grade, and in math between 8th and 10th grade. The sizes of the effects are relatively modest, but non negligible.

The results are in line with the expectations of the literature. Early selection is assumed to have an inequality increasing effect due to different teacher quality and due to different peer effects between the different tracks. The longer a child studies in a selected track, the higher his/her test score gain will be compared to student gains in shorter tracks. This is what I observe in the Hungarian case.

This paper also shows that students, who are left in general schools in areas where the best students can opt-out to early-selective tracks, perform worse in mathematics, than similar students in general tracks with no option of leaving.

In sum I conclude that early selection in Hungary increases differences between students of different status and thus add to the not-small inequality of opportunity of the Hungarian education system.

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APPENDIX

(figure A.1 and A.2 around here)

(table A.1 around here)

TABLES

Table 1 – The official NABC database

	4th grade	**6th grade**	**8th grade**	**10th grade**
2003	0	20 students from every school	0	20 students from each track from each school
2004	0	20 students from every school	20 students from every school	20 students from each track from each school
2006	full cohort	every student from a sample of 195 schools	full cohort	30 students from each track from each teaching site
2007	full cohort	every student from a sample of 200 schools	full cohort	30 students from each track from each teaching site
2008*	every student from a sample of 200 schools	full cohort	full cohort	full cohort
2009*	every student from a sample of 200 schools	full cohort	full cohort	full cohort
2010*	every student from a sample of 200 schools	full cohort	full cohort	full cohort

* Permanent individual identification numbers are available

Table 2– Number (and percentage) of students in different tracks, 2008/6th – 2010/8th

		2010/8th grade				
2008/6th grade	Type of track	General	8-yr-ac	6-yr-ac	Missing (dropout/repeat)	Total
	General	91200	194	5318	6821	103533
		(81,9%)	(0,2%)	(4,8%)	(6,1%)	(93,0%)
	8-yr-ac	136	3839	80	66	4121
		(0,1%)	(3,4%)	(0,1%)	(0,1%)	(3,7%)
	Missing (dropout/repeat)	3331	42	126	149	3648
		(3,0%)	(0,0%)	(0,1%)	(0,1%)	(3,3%)
	Total	94667	4075	5524	7036	111302
		(85,1%)	(3,7%)	(5,0%)	(6,3%)	(100,0%)

Table 3 – Number (and percentage) of students in different tracks, 2008/8th – 2010/10th

		2010/10th grade						
2008/8th grade	Type of track	8-yr-ac	6-yr-ac	4-yr-ac	techn.	vc. training	Missing (dropout /repeat)	Total
	General	139	293	28715	35948	17281	16097	98473
		(0,1%)	(0,2%)	(23,2%)	(29,0%)	(13,9%)	(13,0%)	(79,4%)
	8-yr-ac	3024	32	488	191	23	177	3935
		(2,4%)	(0,0%)	(0,4%)	(0,2%)	(0,0%)	(0,1%)	(3,2%)
	6-yr-ac	20	4617	599	249	24	277	5786
		(0,0%)	(3,7%)	(0,5%)	(0,2%)	(0,0%)	(0,2%)	(4,7%)
	Missing (dropout/ repeat)	83	220	2627	6716	5979	153	15778
		(0,1%)	(0,2%)	(2,1%)	(5,4%)	(4,8%)	(0,1%)	(12,7%)
	Total	3266	5162	32429	43104	23307	16704	123972
		(2,6%)	(4,2%)	(26,2%)	(34,8%)	(18,8%)	(13,5%)	(100,0%)

Table 4 – Test scores means and standard errors by track type and grade

Math	2008/6th grade	2010/8th grade	2008/8th grade	2010/10th grade
General (other)	1491	1615	1604	1615*
	(0,67)	(0,70)	(0,66)	(0,73)
8-yr-ac	1673	1782	1772	1821
	(2,74)	(3,06)	(2,96)	(3,52)
6-yr-ac	1675	1756	1755	1807
	(2,42)	(2,66)	(2,49)	(2,96)
4-yr-ac			1693*	1704
			(1,01)	(1,15)
technikum			1598*	1608
			(0,85)	(0,94)
voc. train.			1450*	1452
			(1,24)	(1,29)
Read	2008/6th grade	2010/8th grade	2008/8th grade	2010/10th grade
General	1494	1576	1583	1628*
	(0,67)	(0,67)	(0,66)	(0,75)
8-yr-ac	1667	1746	1754	1817
	(2,42)	(2,44)	(2,57)	(2,78)
6-yr-ac	1661	1726	1739	1806
	(2,15)	(2,18)	(2,17)	(2,32)
4-yr-ac			1682*	1744
			(0,93)	(0,98)
technikum			1578*	1622
			(0,82)	(0,89)
voc. train.			1408*	1410
			(1,31)	(1,48)

Note: Starred numbers are generated using the panel structure of the data. No such tracks exist in the given years. Standard errors in parentheses

Table 5 – SES index by track type and grade

SES	2008/6th grade	2008/8th grade
General (other)	-0,059	
8-yr-long	0,85	0,8
6-yr-long	0,82	0,8
4-yr-ac		0,39
technikum		-0,1
voc. train.		-0,81

Table 6 – 6-yr-ac track choice, logit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES				6-yr-ac			
Standardized mean score (math & read) 6th grade		3.889***	2.884***	2.788***	2.927***	4.596***	2.957***
		(0.287)	(0.199)	(0.192)	(0.211)	(0.166)	(0.217)
SES	2.961***		2.039***	1.850***	1.947***	2.252***	1.820***
	(0.161)		(0.0919)	(0.0846)	(0.109)	(0.0757)	(0.104)
score * SES					0.927	0.895***	0.937
					(0.0455)	(0.0276)	(0.0477)
female	1.189***	1.036	1.089	1.100	1.099	1.097**	1.106
	(0.0758)	(0.0672)	(0.0695)	(0.0708)	(0.0706)	(0.0408)	(0.0698)
distance bw. home and closest 6-yr-ac (km)				0.930***	0.930***	0.972***	0.978***
				(0.00528)	(0.00528)	(0.00633)	(0.00668)
Constant	0.0339***	0.0317***	0.0257***	0.0429***	0.0421***		0.0525***
	(0.00359)	(0.00352)	(0.00295)	(0.00510)	(0.00509)		(0.00647)
School-site FE	n	n	n	n	n	y	n
Observations	94,024	93,863	89,506	89,352	89,352	48,057	48,057
Number of school-sites						1,162	

Robust site-clustered se in parentheses, ORs reported, *** p<0.01, ** p<0.05, * p<0.1

Table 7 – Track differences in value added, 2008/6th - 2010/8th grade panel, OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Read	Math	Read	Math	Read	Math	Read	Math	Read	Math
General school	-0.768***	-0.696***	-0.144***	-0.00815	-0.0327**	0.0495**	-0.0383***	0.0221	-0.201***	-0.128
	(0.0353)	(0.0509)	(0.0152)	(0.0215)	(0.0139)	(0.0211)	(0.0145)	(0.0208)	(0.0398)	(0.0886)
8-yr-ac	0.101**	0.128*	0.0775***	0.138***	0.0759***	0.136***	0.0732***	0.138***	0.00458	0.158
	(0.0490)	(0.0754)	(0.0202)	(0.0321)	(0.0186)	(0.0312)	(0.0188)	(0.0304)	(0.0598)	(0.0998)
Standardized reading score, 6th grade			0.778***		0.565***	0.165***	0.565***	0.165***	0.555***	0.174***
			(0.00413)		(0.00496)	(0.00555)	(0.00496)	(0.00555)	(0.00414)	(0.00401)
Standardized math score, 6th grade				0.769***	0.230***	0.630***	0.230***	0.630***	0.249***	0.624***
				(0.00559)	(0.00492)	(0.00644)	(0.00493)	(0.00645)	(0.00367)	(0.00488)
SES					0.0825***	0.0579***	0.0832***	0.0597***	0.0675***	0.0644***
					(0.00380)	(0.00441)	(0.00379)	(0.00441)	(0.00277)	(0.00282)
female					0.222***	-0.131***	0.222***	-0.131***	0.222***	-0.136***
					(0.00493)	(0.00537)	(0.00493)	(0.00537)	(0.00470)	(0.00471)
Education provider FE.	n	n	n	n	n	n	y	y	n	n
School-site FE	n	n	n	n	n	n	n	n	y	y
Constant	0.757***	0.681***	0.108***	-0.0180	-0.106***	-0.00754	-0.100***	0.0227	0.0480	0.154*
	(0.0342)	(0.0501)	(0.0148)	(0.0210)	(0.0138)	(0.0211)	(0.0148)	(0.0215)	(0.0367)	(0.0835)
Observations	85,550	85,549	85,550	85,549	82,211	82,210	82,211	82,210	82,211	82,210
R-squared	0.062	0.051	0.598	0.577	0.638	0.595	0.638	0.596	0.696	0.711

Robust school-site clustered standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8 –Track differences in value-added, 2008/8th - 2010/10th grade panel, OLS

VARIABLES	(1) Read	(2) Math	(3) Read	(4) Math	(5) Read	(6) Math	(7) Read	(8) Math	(9) Read	(10) Math
8-yr-ac	0.0577	0.0728	0.00593	0.00913	-0.00340	0.00828	-0.00647	0.00782	-0.000953	-0.00652
	(0.0491)	(0.0761)	(0.0261)	(0.0345)	(0.0226)	(0.0334)	(0.0224)	(0.0318)	(0.0321)	(0.0395)
4-yr-ac	-0.300***	-0.510***	-0.116***	-0.284***	-0.0894***	-0.235***	-0.0871***	-0.244***	-0.0830***	-0.234***
	(0.0362)	(0.0515)	(0.0194)	(0.0234)	(0.0170)	(0.0215)	(0.0170)	(0.0211)	(0.0206)	(0.0225)
Standardized reading score, 8th grade			0.620***		0.499***	0.179***	0.499***	0.178***	0.465***	0.154***
			(0.00489)		(0.00484)	(0.00511)	(0.00485)	(0.00509)	(0.00430)	(0.00441)
Standardized math score, 8th grade				0.676***	0.164***	0.534***	0.162***	0.531***	0.132***	0.491***
				(0.00811)	(0.00460)	(0.00823)	(0.00459)	(0.00816)	(0.00385)	(0.00749)
SES					0.0555***	0.0558***	0.0567***	0.0572***	0.0272***	0.0335***
					(0.00363)	(0.00403)	(0.00361)	(0.00398)	(0.00265)	(0.00293)
female					0.155***	-0.366***	0.154***	-0.367***	0.130***	-0.358***
					(0.00682)	(0.00755)	(0.00679)	(0.00753)	(0.00545)	(0.00628)
technikum	-0.897***	-0.986***	-0.362***	-0.415***	-0.273***	-0.359***	-0.262***	-0.367***	-0.247***	-0.342***
	(0.0394)	(0.0579)	(0.0213)	(0.0270)	(0.0190)	(0.0243)	(0.0192)	(0.0242)	(0.0259)	(0.0296)
voc. Training	-1.935***	-1.763***	-0.841***	-0.657***	-0.676***	-0.546***	-0.669***	-0.558***	-0.628***	-0.558***
	(0.0413)	(0.0565)	(0.0254)	(0.0297)	(0.0238)	(0.0278)	(0.0239)	(0.0278)	(0.0296)	(0.0345)
ed. Provider FE	n	n	n	n	n	n	y	y	n	n
School-site FE	n	n	n	n	n	n	n	n	y	y
Constant	0.826***	0.894***	0.338***	0.374***	0.187***	0.502***	0.199***	0.536***	0.182***	0.497***
	(0.0352)	(0.0539)	(0.0188)	(0.0234)	(0.0172)	(0.0224)	(0.0184)	(0.0229)	(0.0217)	(0.0243)
Observations	76,530	76,521	76,530	76,521	73,256	73,253	73,256	73,253	73,256	73,253
R-squared	0.375	0.255	0.639	0.592	0.659	0.633	0.660	0.634	0.699	0.677

Robust school-site clustered standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 9 – Track differences in value-added, 2008/6th – 2010/8th grade panel, 8-yr-ac vs. 6-yr-ac tracks, 2*SLS*

	1st stage	2nd stage		1st stage	2nd stage	
	All settlements			**Settlements where both tracks available**		
VARIABLES	8-yr-ac	Read	Math	8-yr-ac	Read	Math
8-yr-ac		0.0927**	0.169***		0.113	0.314***
		(0.0435)	(0.0635)		(0.0743)	(0.102)
distance bw. home and closest 8-yr-ac	-0.0194***			-0.0193***		
	(0.00152)			(0.00269)		
distance bw. home and closest 6-yr-ac	0.0176***			0.0139***		
	(0.00160)			(0.00355)		
Standardized reading score, 6th grade	0.0100	0.545***	0.189***	0.0167	0.533***	0.176***
	(0.0137)	(0.0118)	(0.0138)	(0.0181)	(0.0146)	(0.0160)
Standardized math score, 6th grade	-0.0194	0.218***	0.701***	-0.0195	0.224***	0.719***
	(0.0181)	(0.0105)	(0.0135)	(0.0237)	(0.0118)	(0.0158)
SES	-0.00682	0.0482***	0.0751***	-0.0113	0.0505***	0.0721***
	(0.0112)	(0.00839)	(0.00872)	(0.0156)	(0.00917)	(0.0107)
female	0.0210	0.190***	-0.242***	0.0229	0.194***	-0.241***
	(0.0165)	(0.0137)	(0.0142)	(0.0214)	(0.0154)	(0.0172)
Constant	0.443***	-0.0281	-0.0217	0.421***	-0.0400	-0.0825*
	(0.0586)	(0.0261)	(0.0379)	(0.0715)	(0.0354)	(0.0486)
ed. provider FE	y	y	y	y	y	y
Observations	7,986	7,989	7,986	5,878	5,881	5,878
R-squared	0.209	0.536	0.646	0.064	0.522	0.636

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10 – Track differences in value-added, 2008/8th – 2010/10th grade panel, 8-yr-ac vs. 6-yr-ac tracks, 2SLS

	1st stage	2nd stage		1st stage	2nd stage	
		All settlements		**Settlements where both tracks available**		
VARIABLES	8-yr-ac	Read	Math	8-yr-ac	Read	Math
8-yr-long		0.0491	0.0942*		0.238**	0.0725
		(0.0474)	(0.0510)		(0.0936)	(0.0965)
distance bw. home and closest 8-yr-ac	-0.0174***			-0.0185***		
	(0.00166)			(0.00274)		
distance bw. home and closest 6-yr-ac	0.0175***			0.0168***		
	(0.00216)			(0.00363)		
Standardized reading score, 8th grade	0.00938	0.458***	0.117***	0.0180	0.444***	0.113***
	(0.0162)	(0.0128)	(0.0132)	(0.0206)	(0.0163)	(0.0149)
Standardized math score, 8th grade	0.00955	0.212***	0.756***	0.00760	0.221***	0.755***
	(0.0199)	(0.0118)	(0.0136)	(0.0249)	(0.0155)	(0.0158)
SES	-0.0245*	0.0311***	0.0393***	-0.0384**	0.0380***	0.0406***
	(0.0133)	(0.00895)	(0.00941)	(0.0171)	(0.0115)	(0.0110)
female	0.00678	0.177***	-0.296***	0.00971	0.179***	-0.281***
	(0.0186)	(0.0141)	(0.0157)	(0.0236)	(0.0174)	(0.0185)
Constant	0.424***	0.182***	0.354***	0.413***	0.109**	0.365***
	(0.0594)	(0.0294)	(0.0302)	(0.0696)	(0.0474)	(0.0466)
ed. Provider FE	y	y	y	y	y	y
Observations	6,375	6,379	6,375	4,782	4,785	4,782
R-squared	0.189	0.500	0.687	0.059	0.464	0.678

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11 – Track differences in value-added, 2008/6th – 2010/8th grade panel, 6-yr-ac vs. general tracks, 2SLS

	1st stage	2nd stage		1st stage	2nd stage	
	Education provider fixed effects			**School-site fixed effects**		
VARIABLES	6-yr-ac	Read	Math	6-yr-ac	Read	Math
6-yr-ac		0.466**	0.115		0.249	10.09
		(0.215)	(0.301)		(7.394)	(9.202)
distance bw. home and closest 8-yr-ac	0.000633*			6.74e-05		
	(0.000333)			(6.22e-05)		
distance bw. home and closest 6-yr-ac	-0.00226***			-7.58e-05		
	(0.000271)			(7.25e-05)		
Standardized reading score, 6th grade	0.0103***	0.562***	0.163***	0.000883	0.562***	0.167***
	(0.00184)	(0.00553)	(0.00632)	(0.000572)	(0.00725)	(0.00902)
Standardized math score, 6th grade	0.0284***	0.217***	0.622***	0.00154***	0.253***	0.609***
	(0.00363)	(0.00815)	(0.0111)	(0.000568)	(0.0118)	(0.0147)
SES	0.0208***	0.0738***	0.0562***	0.000389*	0.0656***	0.0619***
	(0.00239)	(0.00595)	(0.00810)	(0.000205)	(0.00381)	(0.00474)
female	0.00626**	0.219***	-0.128***	-0.000126	0.225***	-0.129***
	(0.00285)	(0.00536)	(0.00589)	(0.000254)	(0.00414)	(0.00515)
Constant	0.0454***	-0.152***	0.0381***	0.0570***	-0.160	-0.543
	(0.00689)	(0.00886)	(0.0119)	(0.000768)	(0.422)	(0.525)
ed. provider FE	y	y	y	n	n	n
school-site FE	n	n	n	y	y	y
Observations	78,744	78,743	78,744	82,051	82,052	82,051
R-squared	0.187	0.621	0.583	0.002		
Number of school-sites				2,762	2,762	2,762

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12 – Track differences in value-added, 2008/8th – 2010/10th grade panel, 6-yr-ac vs. general tracks, 2SLS

	1st stage	2nd stage		1st stage	2nd stage	
	Education provider fixed effects			**School-site fixed effects**		
VARIABLES	6-yr-ac	Read	Math	6-yr-ac	Read	Math
6-yr-ac		-0.0434	0.225**		-0.252	0.0667
		(0.103)	(0.102)		(0.157)	(0.170)
distance bw. home and closest 8-yr-ac	0.000452			-0.00109***		
	(0.000647)			(0.000353)		
distance bw. home and closest 6-yr-ac	-0.00592***			-0.00202***		
	(0.000554)			(0.000391)		
Standardized reading score, 6th grade	0.0252***	0.503***	0.169***	0.0166***	0.472***	0.143***
	(0.00495)	(0.00807)	(0.00816)	(0.00391)	(0.00574)	(0.00620)
Standardized math score, 6th grade	0.0185***	0.192***	0.646***	0.0139***	0.163***	0.613***
	(0.00659)	(0.00655)	(0.00987)	(0.00446)	(0.00524)	(0.00567)
SES	0.0347***	0.0579***	0.0475***	0.0226***	0.0349***	0.0303***
	(0.00453)	(0.00568)	(0.00627)	(0.00302)	(0.00542)	(0.00586)
female	-0.0137*	0.139***	-0.358***	-0.00239	0.127***	-0.358***
	(0.00708)	(0.00827)	(0.00879)	(0.00433)	(0.00664)	(0.00718)
Constant	0.132***	0.114***	0.238***	0.143***	0.183***	0.277***
	(0.0163)	(0.0160)	(0.0153)	(0.00524)	(0.0185)	(0.0200)
ed. provider FE	y	y	y	n	n	n
school-site FE	n	n	n	y	y	y
Observations	28,354	28,353	28,354	28,577	28,576	28,577
R-squared	0.084	0.531	0.633	0.020		
Number of school-sites				512	512	512

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13 – Propensity score matching, number of treated and controls

		Nearest neighbor	Stratification	
kilometers from nearest early-selective track	15	34438	34438	Treated
		11596	16119	Control
	20	34438	34438	Treated
		5913	6679	Control
	25	34438	34438	Treated
		2546	2642	Control

Table 14 – Propensity score matching, average treatment effects

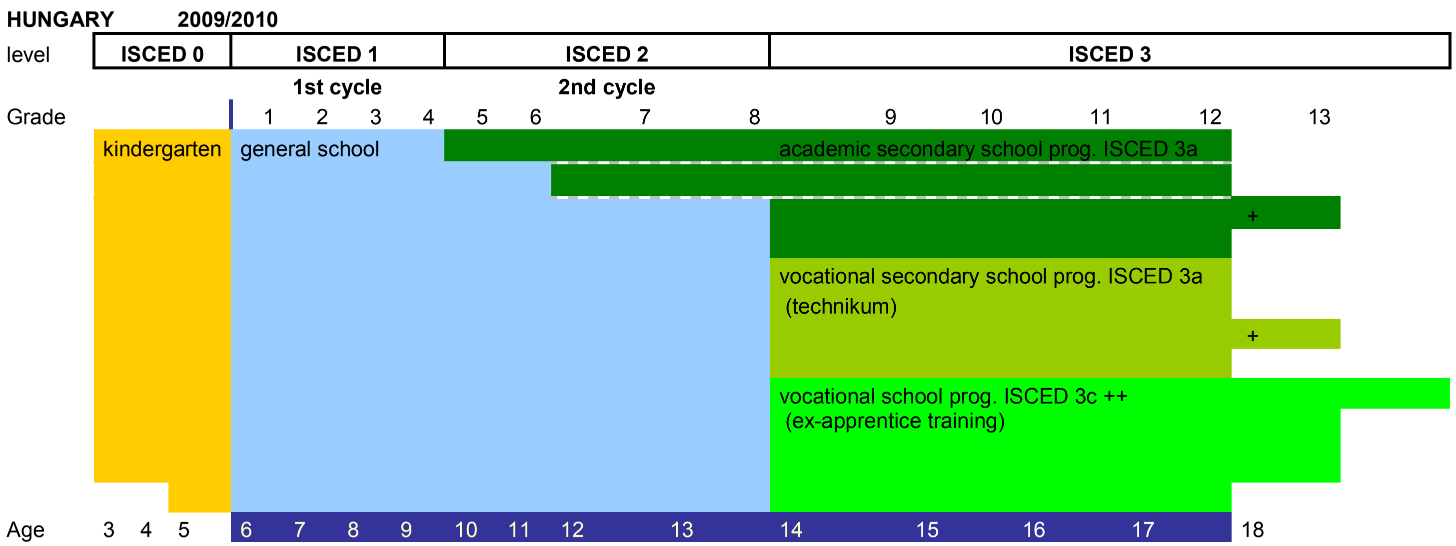
		Nearest neighbor	Stratification	
	Mathematics			
kilometers from nearest early selective track	15	**-0,034**	**-0,032**	Mean
		(0,013)	(0,009)	(se)
	20	**-0,045**	**-0,030**	Mean
		(0,018)	(0,012)	(se)
	25	-0,038	-0,032	Mean
		(0,028)	(0,020)	(se)
	Reading			
	15	0,000	-0,003	Mean
		(0,013)	(0,008)	(se)
	20	-0,007	0,004	Mean
		(0,018)	(0,011)	(se)
	25	-0,023	0,005	Mean
		(0,028)	(0,016)	(se)

Note: significant effects in bold.

Table A.1 – Track type combinations within school-sites, 2010

Number of sites	general	4-yr-ac	6-yr-ac	8-yr-ac	technikum	voc. train.
2463	+	-	-	-	-	-
228	-	-	-	-	+	+
162	-	-	-	-	+	-
95	-	+	-	-	+	-
91	-	+	-	-	-	-
82	+	+	+	-	-	-
78	-	-	-	-	-	+
72	+	+	-	-	-	-
52	+	+	-	+	-	-
50	-	+	-	-	+	+
43	+	-	-	-	-	+
23	+	-	-	+	-	-
23	+	-	+	-	-	-
23	+	+	+	-	+	-
15	+	+	-	+	+	-
14	+	-	-	-	+	-
8	-	+	+	-	-	-
8	+	-	-	-	+	+
8	+	+	-	-	+	-
7	+	+	-	-	-	+
7	+	+	-	-	+	+
5	-	+	-	-	-	+
3	+	-	+	-	+	-
3	+	+	+	+	-	-
2	-	+	-	+	-	-
2	-	+	+	-	+	-
2	+	+	+	-	+	+
1	-	+	+	-	+	+
1	+	-	-	+	+	-
1	+	-	+	+	-	-
1	+	+	-	+	-	+
1	+	+	-	+	+	+
1	+	+	+	+	+	-

Figure A.1 – The Hungarian compulsory education system



compulsory education until the age of 18 applies for the 1st graders in 1998 and later (previously: until the age of 16)

vocational secondary school programmes curriculum includes vocational subjects and many students progress to PS voc to get a VQ

+ : some schools offer an extra grade teaching a foreign language before secondary school educ. (i.e. between grade 8 and 9)

++: some programmes are also available for elementary school drop-outs

ISCED	english	national language	share
0	kindergarten	óvoda	
1,2a	general school	általános iskola	100%
3a	secondary grammar school prog.	gimnázium	
3a	vocational secondary school prog.	szakközépiskola	
3c	vocational school prog.	szakiskola	

Figure A.2 – Spatial distribution of early selective tracks in Hungary, small-regions (LAU1 level)